other in that of the British Islands, the former being stationary, the latter in motion; and as it is said now to be Siberia, its motion must have been from west to east. On the other hand, we find this motion described as being in the opposite direction, viz., from east to west. Sir George Airy, in his treatise on Magnetism, p. 93, remarks "that at Greenwich the dip and total force are diminishing. Interpreting these by the remarks, it would seem that the magnetic equator is approaching above Greenwich, or the north magnetic pole is receding from Greenwich; and remarking also the westerly change in direction of north magnetic meridian, from the sixteenth century to the year 1824, and its subsequent easterly motion, it would seem that the north magnetic pole has rotated round the terrestrial pole in a small circle from east to west, and, having passed the point where its westerly azimuth, as viewed from Greenwich, is maximum, is still continuing its course in that circle. It seems probable that in the fifteenth or sixteenth century it was situated between North Cape and Spitzbergen. It is now north-west of Hudson's Bay."

The north magnetic pole, which Sir E. Sabine supposes to have been in Halley's time in the meridian of the British Islands, would appear to be the same which Sir G. Airy says was probably in the 13th and 16th century in a meridian between North Cape and Spitzbergen; yet the pole referred to is in one case said to be now in Siberia; while in the other it is said to be to the north-west of Hudson's Bay; but it cannot at the same time have gone to the eastward and also to the westward.

In the two accounts there is a discrepancy, but perhaps this is apparent only, and some of your readers may be able to show how the accounts can be reconciled. X

Height of Thunderclouds

A FEW days ago I had an opportunity of estimating the height of a bank of thunderclouds, an account of which may interest the readers of NATURE.

I was camped at Gurpur, a place some eight miles from and within sight of the sea, with an elevation of about 480 feet. The evening was fine, and the horizon to westward remarkably free from haze, so much so that when the sun dipped it was still too bright for the naked eye to bear. Some fifteen or eighteen miles to eastward a heavy thunderstorm was raging, and the Western Ghâts were shrouded by immense masses of cumulus, which, piled up to an enormous height, and rosy with the beams of the setting sun, formed quite a study for an artist.

Having been in the jungles for three or four weeks, I noted, in order to get correct time, the moment the sun disappeared beneath the horizon. This was, by my watch, six minutes past six o'clock. A few minutes subsequently I noticed the earth-shadow creeping up the clouds to eastwards, its edge being singularly well-defined by the contrast of the cold grey beneath and the warm colouring above. Struck by the slow progress of the shadow, I timed it, and found that at seventeen minutes past six the last tinge of pink faded from the highest point of the cumulus, and at nineteen and a quarter minutes the cirrhi floating above the storm lost their hue, thus giving eleven minutes for the former and thirteen and a quarter for the latter. These times reduced and corrected for latitude (13° N.), give the respective approximate heights of the clouds as 14,075 and 25,590 feet, or, adding height of observer, about 2·75 and 4·93 miles.

To be on the safe side, but 10′ of arc have been allowed for the eastward position of the clouds. The nature of the observations of course renders correction for refraction unnecessary, so that the above figures are well within the mark.

I believe that in the tropics cumuli attain a considerably greater elevation than is generally believed. In 1864 I was on board a vessel in lat. 2° 53′ N., long. 10° 47′ W., when there were constant flashes of forked lightning visible among detached clouds directly overhead, yet not the faintest growl of thunder was heard by anyone on board, although a dead calm prevailed at the time. This fact I can only attribute to the combined effect of the immense altitude of the clouds and the consequent rarity of the air. E. H. PRINGLE

Mangalore, South Canara, Nov. 2

PERIODICITY OF RAINFALL

HAVING been working at the above subject for the last ten years, it occurs to me that a brief record of my failures and successes will form an appropriate supplement to the important article by Mr. Lockyer in NATURE for Dec. 12.

Meteorologists have been hunting for a Saros throughout the present century. Among them, perhaps, the most devoted to the subject were Lieut. George Mackenzie, author of "The System of the Weather," and Luke Howard, whose "Cycle of Eighteen Years in the Seasons of Britain" is a well-known work.

What little I have done in the subject is briefly told. Almost immediately after commencing the collection of British rainfall statistics, which has now reached a completeness excelling that of any other country, my attention was naturally drawn to the question of periodicity. Knowing, however, something of the care requisite to obtain long series of observations strictly comparable, I waited five years before printing anything bearing upon it; in 1865, however, I prepared and published * the following table for fifty years, based upon the mean of continuous records in different parts of Great Britain:—

TABLE I.—MEAN DEPTH OF RAIN AT TEN STATIONS, 1815—1864

Year.	Depth.	Year.	Depth.	Year.	Depth.	Year.	Depth.	Year.	Depth.
1815	27·12	1825	26·57	1835	28·56	1845	27·87	1855	23·37
1816	29·26	1826	23·76	1836	33·49	1846	29·57	1856	25·89
1817	29·73	1827	29·53	1837	24·54	1847	25·80	1857	25·70
1818	30·34	1828	33·02	1838	27·11	1848	35·98	1858	22·79
1819	30·46	1829	28·70	1839	31·27	1849	28·51	1859	28·53
1820	24·53	1830	30·83	1840	24·67	1850	26·35	1860	33·34
1821	29·92	1831	32·28	1841	33·51	1851	26·70	1861	26·98
1822	26·63	1832	26·20	1842	25·53	1852	35·53	1862	30·37
1823	31·09	1833	29·71	1843	30·40	1853	27·38	1863	26·93
1824	30·91	1834	24·52	1844	23·72	1854	22·38	1864	22·11
Mean.	28·999		28·512		28·280		28·607		26·601

I also called attention to two features in this table, which strongly tend towards the confirmation of Mr. Meldrum's views, viz.:—(1) that the wettest years are 1836, 1841, 1848, 1852, and 1860; (2) that of these, all but two form a 12-year period, viz., 1836, 1848, 1860, to which we may now add 1872 †; (3) that the dry years were 1826, 1834, 1844, 1854, 1855, 1858, and 1864; (4) that of these, all but three form a 10-year period, viz., 1834, 1844, 1854, and 1864.

All this looked very satisfactory; but, to make assurance doubly sure, I determined to make up a longer period. This I accordingly did; and the approximate fluctuation of annual rainfall during *one hundred and forty years*, viz., 1726 to 1865, will be found in the British Association Report for 1866, page 286, *et seq.* These values were converted into ratios, and, subsequently, those for the years 1866 to 1869 were added, and the table was given in the following condensed form in an article on the "Secular Variation of Rainfall in England since 1725," in "British Rainfall, 1870." I was so disappointed at the total disappearance of both the ten- and twelve-year periods, that I cannot say that I have closely scrutinised

TABLE II.—RATIO OF THE FALL OF RAIN IN EACH YEAR SINCE 1725 TO THE MEAN FALL OF SIXTY YEARS, ENDING WITH 1869

Year.	1720	1730	1740	1750	1760	1770	1780	1790	1800	1810	1820	1830	1840	1850	1860
0	...	88	65	61	70	108	75	86	90	100	92	111	89	91	122
1	...	71	58	108	87	70	79	105	96	98	109	108	128	88	92
2	...	83	65	82	71	111	131	117	91	97	100	98	91	138	107
3	...	71	60	87	118	113	93	86	77	92	117	106	110	101	89
4	...	114	89	76	101	129	96	104	85	92	117	90	85	74	73
5	...	102	80	83	82	123	77	84	75	99	96	99	97	88	108
6	109	101	70	100	77	107	107	83	96	107	77	118	108	93	115
7	102	110	95	93	91	89	96	106	94	100	102	87	90	97	103
8	109	70	65	84	128	102	65	88	90	102	120	90	130	80	102
9	97	89	59	81	86	83	116	106	88	99	102	107	98	102	104
Mean	...	89·9	70·6	85·5	91·1	103·5	93·5	96·5	88·2	98·6	103·2	101·4	102·6	95·2	101·5

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* Brit. Assoc. Report, 1865, p. 202.

† See *Times*, Nov. 12 and Dec. 3, 1872.

he values herein given; and as I doubt if better data could be obtained, I do not think that your space would be wasted by reproducing it, and affording your readers the opportunity of detecting any periodicity which may exist.

I now turn to the verification of Mr. Meldrum's conjectured connection between sun-spot and rainfall periodicity. But before giving the results I have obtained, I think it is worthy of consideration whether the total precipitation over the surface of the globe can be expected to be increased by increased cyclonic energy. Increased rainfall surely means increased extraction of moisture from the air, and that involves one of two facts—(1) increased evaporation to supply the increased demand, or (2) rapid and great dessication of the atmosphere. Without expressing a dogmatic or fixed opinion, it certainly seems to me more likely that the effect of cyclones is simply to alter the locality of deposition, than its aggregate amount. Therefore I should by no means hold the connection disproved by series in which exactly opposite results obtain, and I am not myself sanguine as to *direct* proof being forthcoming

TABLE III.—ABSTRACT OF RAINFALL OVER THE GLOBE, 1832-68, ARRANGED ACCORDING TO SUN-SPOT PERIODS.*

	EUROPE.											ASIA.		AFRICA.			AMERICA.					
	BRITISH ISLES			FRANCE.					SWITZERLAND.		ITALY	PALESTINE	INDIA	ALGERIA.			CANADA	UNITED STATES.			S. AMERICA	W. INDIE.
	Guernsey.	Greenwich.	Sandwick, Orkney.	Tarn, Bassin de Saint Ferriol.	Haute Garonne, Toulouse.	Basses Pyrénées, Bages Beost.	Charente Inférieure, Courçon.	Paris.	Geneva.	Great St. Bernard.	Rome.	Jerusalem.	Calcutta.	Algiers.	Oran.	Constantine.	Toronto	Philadelphia.	New York, Fort Columbus.	Massachusetts, New Bedford.	New Granada, Marmato.	Barbadoes
Year.	in.	in.	in.	in.	in.	in.	in.	in.	in	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.	in.
1832		19·3		23·5	21·4		18·9	21·4	20·7	39·8	22·8							39·3		43·8		
1833		23·0		34·2	33·2		22·9	23·4	29·8	73·1	26·0							48·4		37·9		
1834		19·6		24·6	20·4		18·1	18·2	24·4	58·9	13·0							35·1		40·1		
Totals		61·9		82·3	75·0		59·9	63·0	74·9	171·8	61·8							122·8		121·8		
1836		27·1		26·8	23·6		32·4	28·0	27·0	98·0	30·8							43·0	27·6	38·1	100·0	
1837		21·0		23·0	25·6		22·0	24·6	20·7	53·9	25·1							37·1	65·5	34·7	88·4	
1838		23·8		22·3	26·1		31·6	24·1	35·5	93·9	31·4							44·3	41·9	34·0	113·4	
Totals		71·9		72·1	75·3		86·0	76·7	83·2	245·8	87·3							124·4	135·0	106·8	301·8	
1843	37·7	24·6	38·4	29·0	31·9	51·4	30·9	24·1	36·5	34·9	21·3		64·3	30·1	10·9			46·5	41·4	45·0	64·8	45·3
1844	27·7	24·9	32·1	35·3	34·8	52·0	29·8	27·0	34·4	66·1	30·6		73·9	41·2	20·8			38·8	36·4	36·2	88·4	74·5
1845	33·3	22·4	40·9	29·7	32·1	68·6	26·5	26·5	35·7	60·2	38·3		63·1	41·2	24·4			40·3	34·1	42·7	70·5	43·9
Totals	98·7	71·9	111·4	94·0	98·8	172·0	87·2	77·6	106·6	161·2	90·2		201·3	112·5	56·1			125·6	111·9	123·9	223·7	163·7
1847	29·2	17·8	40·8	17·7	19·0	27·3	19·2	19·6	27·3	42·0	31·6	67·6	72·4	51·4	18·4				64·9	40·8		42·5
1848	48·0	30·2	37·8	25·7	30·0	39·6	25·4	24·9	33·5	62·4	25·4	48·2	58·7	40·5	23·1				36·8	36·2		62·8
1849	36·4	23·7	40·5	29·1	23·6	41·0	22·9	26·3	33·9	57·5	20·6	60·0	65·5	22·0	12·6				31·7	32·4		53·0
Totals	113·6	71·7	119·1	72·5	72·6	107·9	67·5	70·8	94·7	161·9	77·6	175·8	196·6	113·9	54·1				133·4	109·4		158·3
1855	30·4	21·1	35·1	31·1	25·6	49·5	28·9	15·7	40·2	46·1	32·3	61·0	70·4	21·5	21·5	16·1	41·6					73·5
1856	34·4	22·2	27·4	44·6	31·9	48·8	27·4	25·0	41·0	44·1	28·3	94·0	64·2	28·7	20·5	15·4	28·1					46·4
1857	31·9	21·4	31·9	31·9	26·8	40·6	31·0	21·5	21·1	22·9	30·2	104·2	69·0	35·4	35·7	24·7	40·6					50·8
Totals	96·7	64·7	94·4	107·6	84·3	138·9	87·3	62·2	102·3	113·1	90·8	259·2	203·6	85·6	77·7	56·2	110·3					170·7
1859	43·4	25·9	44·4	31·0	23·9	28·0	30·0	23·2	26·9	36·7				35·3	15·3	31·5	39·8					55·1
1860	48·0	32·0	38·0	29·3	25·8	40·0	37·0	29·4	41·3	56·3				26·5	18·4	23·0	28·0					60·4
1861	31·2	20·3	41·2	31·0	26·0	23·9	25·9	18·8	30·7	31·7				21·4	13·9	14·9	34·5					71·1
Totals	122·6	78·2	123·6	91·3	75·7	91·9	92·9	71·4	98·9	124·7				83·2	47·6	69·4	102·3					186·6
1866	44·4	30·1	41·6																			
1867	37·1	28·5	39·4																			
1868	34·8	25·2	43·4																			
Totals	116·3	83·8	124·4																			

I do not quite agree with my distinguished friend Mr. Meldrum as to the "disturbing influence of continents." Their outlines and mountain ranges change not, and I am aware of no evidence to show that the annual fluctuation of rainfall is disturbed by continents. Moreover, in the case of some of the smaller islands, we have illustrations in support of the views of Becquerel and the Hon. G. P. Marsh which would completely mask any influence indicated by Mr. Meldrum's statistics. I have no doubt that Mr. Meldrum knows the facts respecting the rainfall in the Isle of Ascension better than I do, and I think that perhaps upon reflection he will be inclined to slightly lessen his claims for the superiority of insular stations.

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* Want of space compels us to omit a similar column referring to the rainfall of Australasia.

I am aware of the rather "heavy" nature of the accompanying table, but the matter is one of much importance and entirely dependent on observed facts, therefore I think you will consider it worthy of the space it will occupy. I have condensed it as much as possible, and have, to the best of my knowledge, selected the most trustworthy and longest continued records at present in my hands.

Having thus placed the data before your readers, it seems undesirable to occupy space with remarks as to my own opinion on the evidence; but I cannot help thinking that it is quite clear that the question must not rest where it is. The evidence is no doubt conflicting; but I cannot think that it is chance alone that has given us (from Table I.):—

Maximum sunspot years	1837	1848	1860	1871 ?
Heavy rainfall ,,	1836	1848	1860	1872
Amount of rainfall	33·49	35·98	33·34	? 34
Per cent. above average	19	28	18	? 20
Minimum sunspot years	1833	1844	1856	1867
Small rainfall ,,	1834	1844	1858	1868
Amount of rainfall	24·52	23·72	22·79	? 28·8
Per cent. below average	13	16	19	+ 2

Almost identical results are given by Table II.

G. J. SYMONS

MAX MULLER ON DARWIN'S PHILOSOPHY OF LANGUAGE *

IN a lecture recently delivered in connection with the Liverpool Literary and Philosophical Society, Prof. Max Müller addressed himself to the phase of Mr. Darwin's theory, which deals with the possibility of the higher animals acquiring the faculty of articulate speech. He first cleared the ground by some general remarks on the previous phases of this old, old controversy touching the origin and destiny of man, referring to the contention between the Materialists and the Idealists, and to the durable impression left upon this controversy by Kant's wonderful "Criticism on Pure Reason," lamenting that Mr. Darwin and his followers should disregard the important conclusions resulting from previous controversies on this subject, and proceed as if their theory of evolution were new. Materialism, he said, is everywhere in the ascendant, while Idealism is almost become a term of reproach. In this riddle of mind and matter, the world is the theatre of a struggle for the primacy of mind over matter. But when the evolutionists contend that the development of the mind of man out of the mind of an animal is a mere question of time, the Professor felt inclined to treat the idea with impatience. Animals must be animals so long as they lack the faculty of abstracting general ideas. Darwin says: "I believe that animals have descended from at most four or five progenitors, and plants from an equal or lesser number. Analogy would lead us one step further, namely, to the belief that all animals and plants have descended from some one prototype. All organic beings have descended from some primordial form into which life was breathed by the Creator." Prof. Max Müller inferred that these four progenitors may be intended for the Radiata, Mollusca, Articulata, and Vertebrata; and said that Mr. Darwin holds firmly that man has been developed from some lower animal, that all animals have been so developed from the lowest to the highest order of organism, and that there is nothing peculiar in man which cannot be explained from germinal seeds or potential faculties existing in lower animals. This question of the descent of man may be called the controversy of the nineteenth century, and requires the whole knowledge of the century to answer it adequately. The lecturer, confining himself to the evolution theory as it affects language, essayed to show that between the language of animals and the language of man there is *no* natural bridge, and that to account for human language such as we possess would require a faculty of which no trace has ever been discovered in lower animals. If, as Mr. Darwin begs us to assume, there were a series of developments graduating insensibly from ape to man, it would of course be impossible to fix a definite point where the ape ended and the man began; but he asks us to assume that which does not exist, and without evidence to support this, of which there is none, the theory remains only a theory. Indeed, said the Professor, whenever the distance between two points in the chain of development seems too great, we are told again and again that we must only imagine a large number of intermitted beings representing gradations insensibly sloping up or sloping down, in order to remove all difficulty. So it is in the case between the monkey and the man. This point was illustrated most appositely by reference to the Hindoo notion that man is descended from the spirit of the Creator, through a series of links now extinct, the first descendant being 9-10ths God and 1-10th man, the second being 8-10ths God and 2-10ths man, and so on till man became 10-10ths man and ceased to be of the essence of the Great Spirit. Mr. Darwin's fallacy, he said, lurks in the very word "development," for the admission of this insensible gradation through a series of organised beings would eliminate not only the difference between ape and man, but likewise the difference between peat and coal, between black and white, between high and low—in fact it would do away with the possibility of all definite knowledge. Mr. Darwin admits that articulate language is peculiar to man, but contends that animals have, in a lower stage of development, the identical faculties necessary to the invention of articulate expressions. To this he replied that no development of mental faculties has ever enabled any animal to connect one single definite idea with one single definite word. He gave various illustrations of the essential difference between the expression of emotions and the expression of ideas or abstract conceptions, and argued at length as to the impossibility of mere emotional signs and sounds developing into articulate speech; and he ridiculed the notion that the materials of language being given, all the rest was a mere question of time, a natural gradation from the neigh of the horse to the poetry of Goethe. Man and animals possess emotional language in common, because man is an animal; but animals do not possess rational language, because they are not man. This distinction between emotional and rational language, so far from being fanciful and artificial, is radical, as proved by various evidence, especially by the testimony of pathology in reference to certain brain diseases. Rational language is to be traced back to roots, and every root is the sign of a general conception or abstract idea of which the animal mind is incapable. Mr. Darwin has said there are savage languages which contain no abstract terms; but the names for common objects, such as father, mother, brother, &c., are abstract terms, and unless Mr. Darwin is prepared to produce a language containing no such names, his statement, said the lecturer, falls to the ground as the result of a misconception of the real nature of a general idea as distinguished from an emotion. This phase of the controversy lies within the Professor's peculiar domain, and he was able to entertain his audience with technical illustrations that in ordinary hands must have proved tedious, but in the hands of the most accomplished linguist of the day proved a source of wonder and amusement to his hearers. He concluded as he had begun, by maintaining that language is the true barrier between man and beast.

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* The following extracts have been forwarded to us by the lecturer, and are taken from the *Liverpool Gazette*.